The Happiness Game

IJoC

In Pursuit of an MA

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In 1950, I submitted "The Happiness Game," my MA thesis, to the Department of Sociology at Columbia University in fulfillment of the requirements for the degree. Having taken this first baby step into academic social science, I was pleased. So was my adviser, Leo Lowenthal. I remember presenting a paper based on the thesis at the annual meeting of the Eastern Sociological Society. It was then laid to rest.

Some sixty years later, as part of his new-found appreciation of American communications research, colleague and friend Paddy Scannell stumbled on the thesis and decided that it was worth resurrecting. He enlisted John Peters and Peter Simonson in this worthy cause, leaving me little choice but to be awe-struck. I am duty-bound, obviously, to respond to this team for proposing that the thesis has something useful to say about the pursuit of happiness from a communications perspective, even though its author—at least at the time—was simply pursuing an MA, not more.

In gratitude to these "overtakers," I will try to accomplish four things: 1) to explain, as best I can, how I decided to embark on graduate study of the sociology of communication in the late 1940s; 2) to recall, as best I can, how I became a student of happiness, so called; 3) to reconsider the thesis, its aims, and methods in the light of the current resurgence of interest in happiness; and 4) to speculate on how the thesis, and its method, may have resurfaced in some of my later work.

"Good Boy" Syndrome

Mysteriously, the first thought that leaped up at me when I tried to tune my mind to the late 1940s was an anecdote—long forgotten—that originated in so-called "motivation research," yet another Viennese import that had invaded American marketing at the time. In a study aiming to explain why certain executives avoided air travel, we were told of a respondent who envisioned a Western Union messenger standing at the doorstep of his home, delivering news of his death in an air crash. Upon reading the message, the respondent 'hears' his wife exclaiming, "The damn fool; I always told him not to fly." The researchers labeled the reluctance of this respondent "fear of posthumous embarrassment."

 I believe that this long-forgotten anecdote is trying to unveil a "good boy" syndrome that may describe my adolescence and young-manhood, and how I joined the ranks of academia. Let's begin in 1944. I had just completed a first semester at Columbia College when I was drafted into the United States Army. Ideologically, I should have volunteered to fight the Nazis, but, instead, when offered a position in

the Army's Japanese Language program, I grabbed it. I spent the rest of the War at, of all places, the University of Chicago, which was to be my first job a decade later. I served seven months as an interpreter in occupied Japan, but rather than enlist for an extended stay in what might have been two years of real adventure, I returned to school when my term of compulsory service was over. In 1947, one of my best friends enlisted in Israel's War of Independence, while I, a committed Zionist, remained an ardent activist on the "home front" in New York! In 1948, with BA in hand, I reviewed my career choices, real and imagined. I had long wanted to be an impresario, actually to manage a circus, but that suddenly seemed wrong for a Sabbath-observing boy from bourgeois Brooklyn, so I spent some time managing a chamber ensemble instead (and much later, a national TV network in Israel). My next thought was to enroll in the Columbia School of Journalism. Although I enjoyed newswriting and researching, and had had great times co-editing the Midwood Argus in high school and then The Geisha Gazette, the 24/7 routine and the smell of scandal seemed too dissonant. So I decided on the next best thing. Having had a taste of the newly-sprouting communications offerings in the graduate sociology department, I decided to *study* journalism rather than perform it, and there were Paul Lazarsfeld, Robert Merton, and Robert Lynd all urging me on. Even then, I continued to commute between Columbia and Brooklyn, putting aside the imagined orgiastic and gossipy weekends of my classmates in favor of family, synagogue, and extramural classes at the Jewish Theological Seminary. I was a good student, in good hands, doing the right things. I made my way through the system correctly and emerged some seven years later from the enriching experience of Columbia's Bureau of Applied Social Research with a wife, a book, a diploma, and a job—and no reason that I can recall for posthumous embarrassment.

His Master's Voice?

Sociology at postwar Columbia was thriving. It drew on the enhanced stature acquired by the social sciences in the service of the war. Rather than having to choose between hiring Paul Lazarsfeld and Robert Merton, Columbia hired both of them. The brightest graduate students flocked to New York and other schools in the Ivy League, their tuition reimbursed by the GI Bill. Lazarsfeld's Bureau offered assistantships and experience in empirical research not only to graduate students, but also to faculty members and qualified young women who acted as project directors. Younger faculty such as Martin Lipset (1996), Herbert Hyman, and others found Columbia equally attractive.

There was another, unique, aspect to Columbia in those days. On the eve of the great war, Lazarsfeld's former associates left Vienna and joined him in New York. Likewise, almost all of the members of the Frankfurt School found their way to Morningside Heights, where they reestablished themselves, their offices, their endowment, and their journal, with the encouragement of the Columbia faculty. As a result, graduate students could choose courses and projects not only with Lazarsfeld, Merton, Lynd, and Lipset, but with luminaries such as Hans Zeisel and Herta Herzog from the Vienna side, and with Frankfurters such as Leo Lowenthal, Siegfried Kracauer, and others. Most of us knew far too little to appreciate the value added by these "refugee intellectuals" until after they disbanded and disappeared.

Good fortune led me to register for a course with Lowenthal during my second graduate year. In Germany, Lowenthal had become interested in biographies, as much for what they revealed about the cultures in which they were embedded as for how these stories were told and why they were so popular.

But I knew nothing of this work and even less, ironically, of Lowenthal's early Judaic scholarship (Lowenthal, 1987). Rather, I was attracted by the prospect of enlisting in a study of listeners' responses to popular radio programs, following Lazarsfeld's methods and Merton's theory. Mail from listeners was underappreciated at the time (and still today), although some Bureau researchers—of whom I was also unaware—thought that it provided insights into listener' lives and the gratifications of listening (Simonson, 2012). As I remember it, Lowenthal was "rehearsing" to become Director of Research at the Voice of America, a post that would have been an irony for a member of the Frankfurt School at any time other than the Cold War. Letters from listeners would soon become one resource, among others, for studying feedback to the VOA. Retrospectively, I detect Lazarsfeld's hand in Lowenthal's appointment. Although he had been in the United States for less than a decade, Lazarsfeld had established excellent connections with the pioneers of survey research and, even more striking, with the leaders of the major broadcast networks, especially Frank Stanton (who rose from Research Director to President of CBS). I believe—but am not certain—that Lazarsfeld helped to land the VOA post for Lowenthal, and in addition, helped to supply Lowenthal with examples of the various genres of radio fan mail. Moreover, together with Berelson, Lazarsfeld had recently (1948) completed the draft of a text on the analysis of communication content, which served as handbook for Lowenthal's team of graduate students. Lowenthal was no novice in content analysis either, given his early work on biographies and his later, much canonized study of "Biographies in Popular Magazines" which appeared, in 1944, in Lazarsfeld and Stanton's edited volume, *Radio Research 1942-1943*.

Lowenthal's study of popular biographies was well known to me. Praised by Merton (1949) as "a rare and successful hybrid of European social theory and American-style empirical social research," I would be proud to say that it served as a model for "The Happiness Game." While there is a certain consonance between the two studies, in that both emphasize an apparent retreat from "achievement orientation"—that is, from "production" to "consumption"—I think that this similarity became apparent only a posteriori, when I attempted to generalize my findings.

Who knows? Maybe the thesis does resonate with its master's voice explicitly or sotto voce. It clearly owes much to the tragic times that brought us together. Lowenthal moved to Berkeley soon after leaving the VOA (and Washington, where he had held various unsatisfactory posts during the War), and we met only several times thereafter. One of my great regrets is having failed to realize, during those Columbia days, how much more I might have learned from him, and how little I was aware of the extent of our shared interests. In spite of the support and warmth that characterized our teacher-student relationship, I daresay, with modesty and regret, that he didn't know much about me, either. We met in his home, but there were only few conversations that went beyond the immediate focus of our project, or the bibliography of his course. But I may be wrong about this; it may be all my fault. I find some (posthumous) consolation in Gertrud Robinson's (2006) overly-kind comparison of our careers.

Happiness Now and Then

In the dissertation, I noted what seemed like a dearth of social science research on happiness, in spite of the American declaration that it is worth pursuing. It may be that I was too unsocialized at the time to assemble a bibliography of happiness studies, although I engaged with the writings of Erich

Fromm and others who had dealt with the subject. I hardly knew enough to credit future colleagues at the Bureau for their work in popular culture and analyses of fan mail. In fact, I was not yet a member of the Bureau.

It all looks quite different today. Nowadays, social scientists, whether economists, political scientists, or psychologists, seem preoccupied with happiness—indeed, an influential group of psychologists has declared, in effect, that they have overdone unhappiness, and have now turned to "positive psychology." For the last few years, the Gallup organization has ranked nations and demographic groups as more or less happy. While I have no intention of entering this debate, nor have I reviewed more than a tiny portion of the output, I am wiser today about how to situate my resurrected work of 60 years ago and pleased that I was able to anticipate some of the issues. To address these issues parsimoniously, I will draw on an exhaustive review of theory and research on "happiness" by Dan Haybron (2011) in the *Stanford Encyclopedia of Philosophy*.

From Haybron's review, it is clear that broadcaster Ted Malone had invited his listeners to report their happiness not as a sense of well-being (the definition preferred by philosophers and others, and similar to what Fromm had been saying), but as a "positive emotional condition," i.e., as a psychological state. Such a state, says Haybron, may be measured "objectively," by means of instruments of all kinds, or "subjectively," by asking respondents themselves. Obviously, members of Malone's audience were being asked to report subjectively about short-run (even momentary) episodes that they had experienced as happy. These useful distinctions are hardly explicated in the thesis, but even after all these years, they contribute clarity to the research, then and now. Following Malone's directions, the letters reported on the emotional arousal that accompanied events of brief duration that were experienced and labeled as "happy."

Haybron's review contributes another useful clarification. It reveals that the thesis is not much concerned with the definition of happiness, even subjective happiness. Rather, it is concerned with those things that are perceived by Malone's listeners as having "caused" or "contributed" to their experiences of whatever it was that they were calling "happy." Intuitively, we think we know why somebody may consider himself happy for having completed the building of a model airplane, or when a spouse remembers a birthday. But Malone (and Katz) did not ask listeners in what that psychological state consists, nor would he have known how to ask. Does the emotion of happiness have a physical aspect? Does it make for a nicer world when one experiences happiness? Does one's own happiness "infect" close associates, as Christaskis and Fowler (2009) have suggested? As far as I can tell, current research is certainly interested in the factors that contribute to happiness, but mostly in the sense of well-being, and less so in the sense of momentary states. Autonomy—choosing one's own tasks—seems to be high on the list of causal agents (but see Atre 2007; Schwartz, 2004); wealth may not be (Kahneman et al., 2006; Lane, 2000).

Fast Forward

Questions about the past dominate this publication. Was Katz aware that he was breaking with the mass society tradition and making room for everyday voices? Did Lowenthal's "Biographies" inspire the distinction between "doing" (production) and "happening" (consumption)? Is there a parallel to be

drawn between the "intimacy" that Katz ascribes to Ted Malone and Merton's analysis of the "pseudo gemeinschaft" of Kate Smith (Simonson, 2004). Maybe. Let's hope so.

Instead of dwelling further on distinguished precursors, let me now conclude with some reflection on how the "Happiness" mail, and particularly its content-analytic method, may have affected my work subsequently. I should add that I had not given thought to this question until this very moment, and I am delighted to discover that the answer is a yes.

I now think, in fact—though I had never reflected on this—that large parts of my later work have to do with close reading of texts, both qualitatively and quantitatively. The most dramatic example of this kind of continuity is evident in the project I conducted with Brenda Danet (Katz & Danet, 1966) on letters, written by new immigrants to customs and immigration officials in Israel, appealing for privileges they had been denied. By analyzing the language of persuasion used in these appeals, we tried to infer how Western-type bureaucracy was perceived by these newcomers from more traditional settings, and to typologize the sorts of appeals that were invoked. Later, we used this methodology to study prayers, i.e., the reasons worshippers offer to God in asking for favors (Katz, Gurevitch, Danet, & Peled, 1969).

Three other studies based on qualitative content analyses might also be mentioned. One of these (Liebes & Katz, 1990) analyzed focus group conversations, cross culturally, on episodes of the nighttime soap opera *Dallas*. Another (Dayan & Katz, 1992) studied the rhetoric of "media events." The third is a line-by-line reading with Christopher Ali and Joohan Kim (Katz et al., in progress) of an essay by Gabriel Tarde (1898) on what we would today call "deliberative democracy."

But the best-example of my interest in close readings of text may have surfaced only lately in a current project, with Menahem Blondheim, on the rhetoric of the Biblical Book of Esther (Katz & Blondheim, in press). At one level, we are examining this curious book to extract a portrait of the communications system of the ancient Persian Empire, as Harold Innis (1950) might have done. At the same time, we are struck by the frequent and ludic use of jumpy verbs, such as to stand, to sit, to bow, to kneel, etc.—but especially the repetitive use of the verb "to fall." This usage, we think, provides a skeleton key to the subtext of the Book that finds consolation in the fall of the evil Haman and his genocidal plot against the Jews of Persia. The gestural verbs also give us the idea that the original might have been a puppet play, with God behind the scenes as the puppet master. So maybe my attraction to content analysis can be traced to childhood training in how to read the Bible.

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